ing, the pictures being delivered the same night. The dynamo is run by a gas-engine; and it was stated that more light could be thus had from gas, than by burning it directly. Sixteen feet of gas per hour will develop one horse-power.

Dr. W. A. Traill had a paper on the Portrush and Giant's Causeway electric tramway; and Mr. H. Smith, one on electric tramways. The former was accompanied by a working model. A review of previously constructed roads was given, and the points of difference emphasized; and the commercial success of the road was announced. Owing to the interest created by this paper, and the first two, Professor Thompson's paper on dynamo-electric machines was left over.

Mr. C. J. H. Woodbury described the 'automatic sprinkler' system in an American mill, and referred to a slow-burning construction of the latter, where heavy beams, widely separated, support a three-inch planking, on which is laid the flooring of hard wood. A large number of sprinklers have been critically compared in the interest of the insurance companies; and the result of this work showed a record favorable to the value of the apparatus, as it had operated in one hundred and forty-one mill fires, without any instance of total failure except in two instances, where the water supply had been shut off from the system. The sprinklers were tested for sensitiveness by exposing them to a jet of steam instead of a fire, because the former is more regular in its action. The resistance of the soldered joints to shearing-stress was exceedingly variable, ranging from twenty-five hundred to seven thousand pounds per square inch.

The first attempts to make sprinklers were devoted to endeavors to construct an arrangement for rigidly holding a valve to a seat; and, after these had proven failures, the method of soldering a cap over the sprinkler was next introduced. Later, Mr. F. Grinnell solved the problem, by placing the valve in the centre of a flexible diaphragm; and the arrangement of the parts was such that the water-pressure kept the valve shut until the soldered joint leaked, and then this same pressure forced the sprinkler open.

Professor Osborne Reynolds discussed the 'friction of journals.' The report of a committee on lubrication was referred to, and various methods of lubrication discussed. The method giving the best results is to let part of the shaft run in a bath of oil, which is then sucked in by the action of the shaft. With oil fed by a siphon or a plain hole, the friction is seven or eight times greater; and, in one experiment, the oil was forced out of the hole with over two hundred pounds pressure on a square inch. Professor Thurston was called upon, and gave his experience with lubricants, confirming the statements of the paper, and referring to a case in which he had used a pump to force oil to the journals. Evidently, if so much friction can be saved by copious and regular oiling, it might pay to supply journals systematically with oil under pressure.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PROCEEDINGS OF THE SECTION OF MATHEMATICS AND ASTRONOMY.

THE first paper read in this section was by Prof. E. C. Pickering, upon the colors of the stars. The need of exact photometric measurement of different parts of their spectra was first pointed out, and the author then described a very ingenious method of accomplishing this. In the telescope tube, a little beyond the focal plane, is a direct vision prism, so set as to give a spectrum extended in declination; and on the preceding side of this prism is placed a piece of plane glass, whose edges are so ground, that, when a small portion of the following side of the cone of rays falls upon it, it gives a small white ghost, just preceding the spectrum and always opposite the same wave-length. In the focal plane is one of Professor Pritchard's neutral-tint wedge photometers, and behind it a thin metal diaphragm with four long narrow slits parallel to the equatorial motion; so that, when the spectrum transits behind them, four little stars — a red, yellow, blue, and a violet — shine through these slits, and the time of the disappearance of each, as they move towards the thicker edge of the wedge, measures its brightness. From these times may be deduced the magnitude and color curve of the star. To fix the same wave-lengths for each observation, the little white ghost is adjusted upon one of two parallel wires, which project out beyond the preceding side of the diaphragm. For a succeeding transit, the ghost is adjusted upon the other wire, half a slit-interval distant, and thus eight points of the spectrum are photometrically measured. Professor Young, of Princeton, spoke very highly of the ingenuity and effectiveness of the device, especially for the systematic measurement of a large number of stars. He pointed out, however, what might be a source of error; viz., the different sensitiveness of different observers' eyes to different colors, so that they would probably observe the times of disappearance of the four colored stars relatively slightly different.

The next paper, by Professor Daniel Kirkwood, discussed the question whether the so-called 'temporary stars' may be variables of long period, referring to the sometimes-claimed identity of the temporary stars of 945 and 1264 with the well-known Tycho Brahe's star, which blazed forth in Cassiopeia in 1572, and whose position is pretty closely known from his measures. The conclusion reached was, that on account of the sudden apparition of the temporary stars, the short duration of their brightness, and the extraordinary length of their supposed periods, they should be considered as distinct from variables.

Professor Mansfield Merriman, the author of the well-known treatise on 'Least squares,' proposed a criterion for the rejection of doubtful observations, founded upon Hagen's demonstration of the law of frequency of error, which was simpler than Pierce's or Chauvenet's. It involves, however, a determination of what is the unit of increment between errors of different sizes, a thing difficult to determine in very many cases. Professor Harkness, of the Naval observatory, thought that in the case of a criterion for the rejection of doubtful observations, — upon which the most eminent mathematicians disagreed, — practically every one was a law unto himself. He noted the rather doubtful method of taking a large number of shots on a target-board as a good illustration of the law of frequency of error, especially in any such case as that of long-distance shooting, where, on account of the varying character of the wind, the skilful marksman will frequently change his rifle-sights an amount corresponding to twenty or thirty feet on the target, and yet make a complete series of bull's-eyes, or very close to it. Professor Rogers, of the Harvard college observatory, expressed his disbelief in the efficacy of least squares to tell the truth, illustrating it by several cases. For rejecting discordant observations, he referred to the late Professor Winlock's method of determining the personal habit and accuracy of each observer as a means of getting an empirical criterion. He closed with an expression of the opinion that the method of least squares was a method of 'covering a multitude of sins.' Professor Pickering said that we must have some criterion, and every one would practically use one of some kind. He referred to his plan of using 'average deviation' as easier to compute than 'probable error,' and considered five times the average deviation a good limit for the rejection of discordant results. Professor Stone, of the University of Virginia, referred to the very common case of only three, four, or five observations of a star, where the data are not sufficient to apply any criterion, and to the advisability, when it was possible, of making more observations to settle the question. Another speaker referred to the importance of a special search for systematic abnormal errors. Professor Rogers referred to the uncertainty of trusting to the impressions upon one's senses, and said that in nine cases out of ten, where he thought he had observed a transit over a particular wire too early or too late, it would come out just the other way. Professor Hough, director of the Dearborn observatory, thought an observer generally incapable of judging or weighting his observations according to his impressions. In the case of uncertain conditions, like an unsteady atmosphere, he thought it best to quit work and wait for better. Professor Frisby, of the Naval observatory, emphasized the danger of rejecting observations, or forming any arbitrary limit for this purpose. Professor Langley, director of the Allegheny observatory, hoped that further experience would be given upon this question of trusting one's own impressions in rejecting or weighting observations, as it was an exceedingly interesting and important one. Professor Merriman, the author of the paper, referred to the importance of eliminating all sources of systematic errors so far as possible, and of separation into groups, for separate discussion, in order to discover such errors. Professor Rogers referred to the various values of the solar parallax which had been deduced in one way or another by least squares, and another speaker referred to the hidden sources of error which least squares could not deal with. Professor Paul, assistant astronomer at the Naval observatory, said the method of least squares was hardly receiving fair treatment in the discussion, and thought the difficulty was that half or three-quarters of those who used the method failed to bear in mind the theory on which it rested: that it only applied to purely accidental errors; whereas in more than half the cases it is actually applied to errors distributed round a point which is continually moving or jumping, due to systematic sources of error or sudden disturbance, and that no attempt is made to discover and eliminate these systematic or sudden-jumping errors, but least squares is applied indiscriminately to the whole, with a sort of blind faith that it will bring good results out of poor observations, and make it all right somehow. He said that, intelligently applied, the method not only gave the most probable result, but furnished the only measure of the exactness of the observations so far as accidental errors were concerned, and at the same time the most effective method of discovering these hidden sources of systematic error. Professor Stone illustrated this by the case of combining many series of comet-observations, made at different observatories, into one orbit, without attempting to discover any systematic errors in the series of the different observers. The discussion was closed by Professor Eddy with remarks upon the necessity of some criterion dependent upon the results themselves, and independent of the observer's arbitrary judgment.

Professor Pickering then read another paper upon systematic errors in stellar magnitudes, showing, without any question, that the magnitudes of all the star-catalogues from that of Ptolemy down to the great work of Argelander in the Durchmusterung — all depending upon eye estimates — are systematically affected by being in, or close to, the Milky Way; they all being estimated too faint, and the error amounting to about half a magnitude in the Milky Way itself. This arises from the brightness of the background upon which the star is viewed. In the Harvard photometry measures, this source of error is avoided; since, in the comparison of each star with the pole-star, the two fields are superposed, and their added brightness affects both stars alike.

Prof. M. W. Harrington, director of the Ann Arbor observatory, read a paper upon the asteroid ring. He showed that the representative average orbit would be an ellipse of small eccentricity, with semi-major axis equal to about 2.7 times that of the earth, and inclined to the plane of the ecliptic about 1°; and that, in the progressive discovery of these small bodies, the average mean distance had gradually increased, but now seemed to have reached its limit. On the assumption that the surfaces of all the aster-

oids have the same reflecting power as Vesta, Professor Harrington reaches the conclusion that the volume of Vesta is about $\frac{5}{17}$ that of all these 230 bodies put together, and that Vesta and Ceres together form almost one-half the total volume.

Professor Rogers of the Harvard college observatory then read two papers. The first one, upon the magnitude of the errors which may be introduced in the reduction of an observed system of stellar co-ordinates to an assumed normal system by graphic methods, showed a great amount of laborious research, and was a good illustration of the vast amount of monotonous work necessary in the present stage of astronomical observation in order to reach the highest degree of accuracy attainable by the search for and elimination of minute systematic errors. His next paper was upon the original graduation of the Harvard college meridian circle *in situ*. This described a method of turning a meridian circle through any desired constant arc up to about 30° without any dependence upon the circle and reading microscopes, effected by means of an arm swinging between fixed stops, and clamping to a circular ring on the axis by an electro-magnetic clamp. With this Professor Rogers claimed to be able to set off a constant arc through as many as five thousand successive movements of the clamping arm. The ingenious method suggested and carried out by Mr. George B. Clark, of the firm of Alvan Clark & Sons, of grinding the clamping circle to a perfect circular form while the telescope was swung round in its Y's, was fully described, and also Professor Rogers's method of arresting the momentum of the telescope at the stops by water-buffer plungers. The great advantage of thus being able to set off a constant arc independent of the circle and microscopes was pointed out, with especial reference to the investigation of division errors and flexure of circle, and also to the division of the circle itself *in situ;* i.e., mounted on its axis and turning on its pivots. Professor Young called attention to the necessity of guarding against expansion and contraction of the bar holding the stops, due to radiation from the observer's body.

Mr. S. C. Chandler, jun., of the Harvard college observatory, gave the results of observations and experiments with an 'almucantar' of four inches aperture, a new instrument devised by Mr. Chandler, which seems to be of remarkable accuracy, and promises to furnish an entirely new and independent method of attacking some of the most important problems in exact observational astronomy. The instrument consists of a telescope and vertical setting-circle, which can be clamped at any zenith-distance, and is supported on a rectangular base which floats in a rectangular trough of mercury, the whole turning round a vertical axis so as to observe in any azimuth; these observations being simply the times of transit of any heavenly body over a system of horizontal wires in the field. The observations thus far have been entirely upon stars, and all at the apparent zenith-distance of the pole. After some very small periodic variations in the zenith-distance pointing had been traced to changes of temperature, and had been removed by sawing through the wooden bottom of the mercury trough, the instrument showed an astonishing constancy in this zenith-distance pointing, extending over weeks at a time, and far exceeding the constancy of the corrections to the best fundamental instruments of our observatories.

A paper was read by Mr. Chandler, upon the colors of variable stars. Showing, first, that most of the variables were *red*, he described some fairly satisfactory methods which he had used to measure the *degree* of redness of all the periodic variables; and then, plotting a series of points whose abscissae represented the *length* of the periods, and ordinates the *degree of redness*, their agreement with a curve making a very decided angle with the axis of abscissae brought out without question the remarkable law, that, *the redder the star, the longer is its period of variability.* In discussing any theory of variable stars, Mr. Chandler pointed out that Zöllner was the only one who had thus far taken into account two laws already known: viz., 1° that they are generally *red;* 2° that they *increase* in brightness *much more rapidly* than they *decrease;* and now, in any further theory, this new third law must have a place, viz., that, the *redder* they are, the *longer* is their period.

Monday's session opened with a paper by Dr. R. S. Ball, astronomer royal of Ireland, upon the ruled cubic surface known as the cylindroid, whose equation is

$$z\,(x^2 + y^2) - 2\,m\,x\,y = 0.$$

Mr. W. S. Auchincloss of Philadelphia exhibited a balancing-machine for finding the centre of gravity of any number of different weights distributed along a line, which seemed to be of excellent construction, extremely easy and rapid in manipulation, and quite sensitive. In connection with a time-scale of three hundred and sixty-five days at one side, it was shown how rapidly a complicated system of business accounts could be settled, and how it could be applied to various engineering problems.

The next paper was by Prof. J. H. Gore, of the U S. geological survey, upon the geodetic work of the U. S. coast and geodetic survey. This was a long paper, much of it devoted to a historical *résumé* of geodetic work in all countries. The points of principal interest brought out were the great advantages possessed by the United States in its vast extent of territory, for determining the figure of the earth; and the work already done along the coasts, and along a chain of triangles from the Atlantic to the Pacific, was shown on a map. The great accuracy attained, especially in base-measurement, was noted, and the great improvements made in apparatus and instruments of the survey. Especially was the importance insisted on of a scientific body like the American association supporting in every way the integrity and unity of this great work. In answer to questions, Professor Gore stated that the most recent improvements in the base-measuring apparatus were the determination of the coefficients of expansion for every degree of temperature to which they would be exposed; and he expressed his belief that results

more accurate still would be attained by immersing them in melting ice, so as to keep them at a constant temperature when in actual use.

The next paper was by Mr. J. N. Stockwell of Cleveland, upon an analysis of the formula for the moon's latitude as affected by the figure of the earth. In this Mr. Stockwell claimed that Laplace's formula for expressing this was wrong; the question turning upon an approximate integration of a differential equation, which he claimed to show was wrong by separating into two terms a single one which expressed the difference of two effects, which, thus evaluated separately, became either indeterminate or of an impossible amount.

Prof. J. C. Adams of Cambridge, England, made some comments upon Mr. Stockwell's paper, the audience eagerly crowding forward that they might lose none of the interesting discussion. Professor Adams spoke in high terms of the general work which Mr. Stockwell had done in the difficult subject of the lunar theory; but, from such conclusions and methods as those brought forward in this particular case, he said he must express his total dissent. He then, in the simple yet forcible manner of a master of mathematical analysis, pointed out that this equation was, to begin with, only an approximation; that, before it could be treated at all as a rigorous one, many other small terms must be included; that, further, its integration was only an approximation; and that in this case, any separation into terms, which, on a certain approximate assumption, became either indeterminate or very large, was of no value as a test of the equation; that, in the case of oculating elements referred to by Mr. Stockwell, these in no sense represented an average orbit, but only an instantaneous state of ever-varying elements; and that any integration proceeding on the first hypothesis, over a long period, would introduce an error increasing with the time which would swallow up entirely the perturbations sought. The celebrated astronomer, than whom neither England nor the whole continent of Europe could have sent one more competent to advise, then closed with a few remarks pregnant with suggestion to workers in the lunar theory, upon the general methods to be followed in these long and difficult solutions by *approximations*. Hearty applause followed; and the animated discussion was brought to a good-natured close, Mr. Stockwell still unconvinced, hoping that when Professor Adams had given more attention to this particular point, he would come to think the same of it as himself; and Professor Adams (amid much laughter) hoping that day would never come.

In Tuesday's session, Professor Ormond Stone, director of the Leander McCormick observatory of the University of Virginia, gave an elaborate description of that observatory now approaching completion, and to be devoted entirely to original research. The telescope, which will soon be mounted, is the twin in size of the Washington twenty-six inch, and like it in most of its details, except the driving-clock, which is like that of the Princeton twenty-three inch, with an auxiliary control by an outside clock, and that it has Burnham's micrometer illumination. The observatory has a permanent fund of seventy-six thousand dollars as a beginning; and eighteen thousand dollars have been expended in observatory buildings, and eight thousand dollars for the house of the director. Situated eight hundred and fifty feet above the sea, and on a hill three hundred feet above surroundings, the main building, circular in shape, is surmounted by a hemispherical dome forty-five feet in diameter. The brick walls have a hollow air-space, with inward ventilation at bottom and outward at top. Mr. Warner, the builder of the dome, gave an interesting description of the ingenious method of adjusting the conical surfaces of the bearing-wheels, so that they would, without guidance, follow the exact circumference of the tracks; and then of the adjustment of the guide-wheels, so that the axis of this cone should be exactly normal to the circular track. The framework of the dome consists of thirty-six light steel girders, the two central parallel ones allowing an opening six feet wide. The covering is of galvanized iron, each piece fitted *in situ*, and the strength of the frame is designed to stand a wind-pressure of a hundred pounds per square foot. There are three equal openings with independent shutters, the first extending to the horizon, the second beyond the zenith, and the third so far that its centre is opposite the division between the first and second. The shutters are in double-halves, opening on horizontal tracks, and connected by endless chain with compulsory parallel motion of the ends. The dome weighs twelve tons and a half, and the live-ring one ton and a half; and a tangential pressure of about forty pounds, or eight pounds on the endless rope, suffices to start it. If this ease of motion continues as the dome grows old, it is certainly a remarkable piece of engineering work.

In the discussion which followed, Professor Hough said that he should prefer the old style of single opening extending beyond the zenith. Professor Stone could not agree with him, the greater extent of opening making it less probable that the dome would have to be moved so far in turning from star to star, and at the same time furnishing better ventilation, and the opportunity for cross-bracing adding strength to the dome. He stated that he should first take up the re-measurement of all the double stars of less than 2″ distance between 0° and −30°.

Father Perry, the director of the observatory at Stonyhurst, Eng., gave the result of late researches on the solar surface, with special reference to evanescent spots. No abstract can give any idea of the wide range of interesting topics covered in this paper. The multitude of ever-changing details to be observed on the sun, and the careful record of these which is kept at the Stonyhurst observatory, furnished the material for a paper replete throughout with new and important details, to which nothing but a publication in full can do any justice whatever; and it is to be hoped that the association will soon give the public the opportunity to read it in this way.

On Wednesday, Mr. Lewis Swift, director of the Warner observatory at Rochester, N.Y., read a paper upon the nebulae, in which he described his method

of search for new nebulae, and of simply recording their approximate positions by pointing with unilluminated cross-wires in the eye-piece, and reading off the circles of the instrument, recording with this a description of the appearance of the nebula. His reason for making no attempt to determine accurate positions was that it would require illuminated micrometer-wires, and a great deal of time devoted to measurement with neighboring stars, besides much time lost in letting the eye become sensitive again for further search or examination after the light was removed; he stating that his eye was practically 'nebula blind' for at least four minutes after being near a light. Since, however, the most of these nebulae are probably too faint to bear any illumination at all, and must therefore be observed for position with ring or bar-micrometer, much of this reason loses its force; for in this case there would be no loss of time on account of light, and if in this way Mr. Swift could connect each of these new nebulae to some neighboring star with the help of chronograph or an assistant at clock or chronometer, and also re-observe the known nebulae in the same way, the value of the work would be almost immeasurably increased compared with the little additional time and labor necessary for its accomplishment. As it is, though no one will deny the value of a catalogue of even the approximate positions and descriptions of very faint nebulae, as a contribution to our knowledge of their number and distribution, and as an aid in comet-seeking or identification, yet it is fairly open to the criticism, that, to be what it should be in the present state of astronomical observation, it must all be gone over again for determinations of accurate positions. One very interesting statement of Mr. Swift, to the effect that there had not been a first-rate clear sky since the red glows appeared a year ago following the Krakatoa explosions, bears out the general experience of workers in other observatories, especially those who try to see stars near the sun in the daytime.

An interesting discussion arose as to the much-disputed existence of the nebula round the star Merope in the Pleiades; the general drift of it being that the nebula no doubt existed, but in order to see it a clear sky was necessary, and a very low power and large field, so that the nebula might be contrasted with darker portions of the same field; that a large telescope was not necessary, in fact the smaller the better, provided the optical qualities were relatively as good. Mr. Swift said he could always see it under favorable conditions; and Mr. E. E. Barnard of Nashville, Tenn., the discoverer of the latest comet, said that before he knew of its existence at all, he picked it up as a supposed comet.

On Thursday Professor Adams of Cambridge, Eng., read a paper upon the general expression for the value of the obliquity of the ecliptic at any given time, taking into account terms of the second order. The difficulties of obtaining a formula for this quantity, on account of the many varying elements upon which it depends, were clearly explained by a diagram, and the results given of an approximation carried much further than ever attempted heretofore.

Professor Harkness, in paying a high compliment to the celebrated mathematician and astronomer for these laborious and valuable researches, also expressed a wish that some of the n-dimensional-space mathematicians would follow the example of Professor Adams, and apply some of their superfluous energy to the unsolved problems in the solar system, which have some direct practical bearing.

Professor Newcomb, in remarking upon the mass of the moon used in this problem, expressed the opinion that this could be obtained most accurately by observations of the sun, in determining the angular value of the radius of the small circle described by the earth about the common centre of gravity of earth and moon, since this, in his opinion, seemed to be the only constant which could be determined by observation absolutely free from systematic errors, and hence was capable of an indefinite degree of accuracy by accumulated observations; and he asked Professor Adams's opinion on this point.

The latter replied, that he thought the quantity too small for *certain* accurate determination, almost beyond what could be actually seen by the eye in the instruments used.

Professor Newcomb admitted, in the case of *absolute* determinations, the general impossibility of attempting to measure what cannot be seen; but, in the case of *differential* or *relative* determinations in which there was no supposed possibility of constant or systematic errors, he advanced the theory, which he had thought of elaborating more fully at some time, that such determinations might be carried by accumulated observations to a sure degree of accuracy far beyond what can be seen or measured by the eye absolutely.

Professor Adams hoped he would more fully elaborate and publish this idea, since there was in it an element well worth careful consideration.

Professor Harkness doubted the sufficient accuracy of meridian observations of the sun, on account of the distortions produced by letting the sun shine full into the instrument; and spoke of the difficulties in the transit-of-Venus observations from this cause.

Professor Newcomb replied that he would have to show that this would be periodic with reference to the moon's quarters in order to affect this constant systematically.

Professor Adams then presented another note upon Newton's theory of atmospheric refraction, and on his method of finding the motion of the moon's apogee. He described in an exceedingly interesting manner how some unpublished manuscripts of the great geometer had lately come into his hands at Cambridge, which contained later work than is published in the Principia. Space will not allow a description of the methods which these papers show that Newton employed in attacking, and remarkably successfully too, some of the problems which still trouble astronomers to-day. Photographs of these papers were exhibited, showing his wonderful neatness and precise methods in computation. It was something of a novelty to those gathered on this occasion, to hold in their hands the facsimiles of the

handwriting and computations of this intellectual giant, whose works will for all time be the greatest wonder to him who studies them the most.

With the hearty thanks of the section to Professor Adams for his exceedingly interesting communications, it was then adjourned.

PROCEEDINGS OF THE SECTION OF PHYSICS.

THE meeting of the American association was one of unusual interest and importance to the members of section B. This is to be attributed not only to the unusually large attendance of American physicists, but also to the presence of a number of distinguished members of the British association, who have contributed to the success of the meetings not only by presenting papers, but by entering freely into the discussions. In particular the section was fortunate in having the presence of Sir William Thomson, to whom more than to any one else we owe the successful operation of the great ocean cables, and who stands with Helmholtz first among living physicists. Whenever he entered any of the discussions, all were benefited by the clearness and suggestiveness of his remarks.

Among the members of the British association who were present, may also be mentioned Professor Fitzgerald of the University of Dublin, Professor Silvanus P. Thompson, Mr. W. H. Preece, superintendent of the English postal telegraph, Professor Forbes, and Professor Schuster of the Cavendish laboratory.

Among American physicists there were Professors Trowbridge, Rowland, Barker, Mendenhall, Hall, Hastings, Bell, Anthony, Brackett, Rogers, Pickering, Cross, and many others. The section was organized on Thursday, Sept. 4, and the opening address delivered by the vice-president, Professor Trowbridge. The time devoted to the reading and discussion of papers was unfortunately much infringed upon by the Electrical conference: yet, considering this serious interruption, the number of interesting discussions was unusually large.

It is not to be expected that the elaborate investigation of the relation of the yard to the metre, such as was the subject of a paper by Professor William A. Rogers, will be of very general interest. Yet to the physicist such a comparison, conducted by one who has had the long experience of Professor Rogers, is of the highest importance in giving accuracy to determinations of length. Professor Rogers has given his life to perfecting the construction and testing of standards of length, and the result of this his latest investigation is that the metre is 39.37027 inches in length. One of the most important physical measurements is that of the wave-length of light of any given degree of refrangibility, and this determination is best made by means of the diffraction grating. On account of the extensive use of the magnificent gratings constructed by Professor Rowland for this purpose, Professor Rogers instituted an investigation to determine the coefficient of expansion of the speculum-metal used in the construction of these gratings. He also noted that from its homogeneity, fineness of grain, and non-liability to tarnish, this speculum-metal is peculiarly suitable for constructing fine scales, though its extreme brittleness is an objection to its use for large scales. Professor Rowland stated that he proposed to construct scales on his ruling-engine which would enable the physicist at any time, by purely optical means, and without knowing the coefficient of expansion of the metal or its temperature, to obtain the value of the length of the scale in terms of the wave-length of any given ray of light. These scales were simply to be straight pieces of speculum-metal ruled with lines just as an ordinary grating, except that the length of the lines is to be only about one centimetre, every one-hundredth line being somewhat longer than its neighbors: the whole ruled strip is to be one decimetre in length. From the manner of ruling, it will be easy to count the whole number of lines in the length of the strip, and then by a simple use of the scale as a grating in a suitable spectrometer the whole length may be immediately found at any time in terms of any specified wave-length of light.

In some forms of telephones and in the microphone, the action depends on the change in resistance of a small carbon button on being subjected to pressure. There has been much discussion as to whether this diminution of the resistance with pressure is due to a change in the resistance of the carbon itself, or simply to the better contact made between the carbon and the metallic conductor when the pressure is applied. Professor Mendenhall has carried out some experiments to determine the question; and one of his methods of experimenting — that with the hard carbons — appears to point conclusively in favor of the theory that the resistance of the carbon itself is altered by pressure. The experiments made by him on soft carbon are open to criticism, though they also point to the change taking place *in* the carbon. Professor Mendenhall finds that the resistance is not simply proportional to the pressure, and thinks that by increasing the pressure a point of maximum conductivity would be reached where there would be no change in resistance for a small change in pressure.

Prof. A. Graham Bell, the inventor of the telephone, read a paper giving a possible method of communication between ships at sea. The simple experiment that illustrates the method which he proposed is as follows: Take a basin of water, introduce into it, at two widely separated points, the two terminals of a battery-circuit which contains an interrupter, making and breaking the circuit very rapidly. Now at two other points touch the water with the terminals of a circuit containing a telephone. A sound will be heard, except when the two telephone terminals touch the water at points where the potential is the same. In this way the equipotential lines can easily be picked out. Now, to apply this to the case of a ship at sea: Suppose one ship to be provided with a dynamo-machine generating a powerful current, and let one terminal enter the water at the prow of the ship, and the other be carefully insulated, except at its end, and be trailed behind